

New Publications

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Items listed in New Publications can be ordered directly from the publisher; they are not available through AGU.

Analytical Chemistry in the Exploration, Mining, and Processing of Materials, L. R. P. Butler (Ed.), Blackwell Scientific, Boston, Mass., vii + 254 pp., 1986, \$30.00.

Atmospheres and Ionospheres of the Outer Planets and Their Satellites, Phys. Chem. Space Ser.,

vol. 15, Sushil K. Atreya, Springer-Verlag, New York, xiii + 224 pp., 1986, \$69.50.

Fjords: Processes and Products, James P. M. Syvitski, David C. Burrell, and Jens M. Skei, Springer-Verlag, New York, x + 379 pp., 1987, \$85.00.

General Circulation of the Ocean, Henry D. I. Abarbanel and W. R. Young (eds.), Springer-Verlag, New York, xii + 291, 1987, \$69.00.

Geophysical Fluid Dynamics, 2nd ed., Joseph Pedlosky, Springer-Verlag, New York, xiv + 710 pp., 1987, \$49.00.

The Physical Nature and Structure of Oceanic Fronts, Lect. Notes Coast. Estuarine Stud. Ser., vol. 19, K. N. Fedorov (Ed.), Springer-Verlag,

New York, viii + 333 pp., 1986, \$53.30 (paperback).

Proceedings of the Tenth Symposium on Antarctic Meteorites 1985, Mem. Natl. Inst. Polar Res. Spec. Iss. Ser., vol. 41, Keizo Yanai, Hiroshi Takeda, and Akira Shimoyama (eds.), National Institute of Polar Research, Tokyo, Japan, v + 393 pp., 1986.

Sediments and Water Interactions, Peter G. Sly (ed.), Springer-Verlag, New York, xxi + 521, 1986, \$105.00.

Thermal Modeling in Sedimentary Basins, Coll. Colloq. Seminares Ser., vol. 44, Jean Burrus (Ed.), Editions Technip, Paris, xix + 600 pp., 1986 ~\$91.00 (paperback).

AGU

1986 James B. Macelwane Awards

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Edward M. Stolper

Citation

I can think of few things more pleasurable than introducing a young scientist whose research has enhanced his visibility to such an extent that his or her image is clearly distinguishable from among the large number of young scientists publishing excellent research these days.

Normally, the recipient of a young scientist award is in a state approaching shock, with mixed feelings of pride and humility and appreciation for all those who guided him or her on the way. For Ed Stolper, however, the situation is different, and he is sitting here quite calmly. Although he is only 33 years old, his image shines brightly enough that it has received attention previously—He was awarded the Clarke Medal of the Geochemical Society in 1985, and he shared the Newcomb Cleveland Prize in 1985 with Sally Rigden and Tom Ahrens for the best 1984 paper in *Science*. Today it is the Macelwane Award of the American Geophysical Union, and there are still several tomorrows before his age disqualifies him as a young scientist, making it necessary for him to start getting

down to serious, mature research.

Before I say any more about his research, permit me to outline a few historical developments. After graduation from Harvard with an A.B. summa cum laude in 1974, with coauthorship on four lunar science papers already to his credit, he won a Marshall Scholarship (some of you know that these are very competitive) and spent 2 years at the University of Edinburgh. Most Marshall Scholars elect to study at Cambridge or Oxford, but Ed went to Edinburgh because he felt that he could learn something from Professor Mike O'Hara, having heard about O'Hara's contributions to the lunar program. This desire to learn has characterized his research career. He returned to Harvard as a Master of Philosophy, became a Doctor of Philosophy by 1979, and was then appointed to a position as an assistant professor without an interval as postdoctoral fellow to organize himself for the real world. But he did not join the real world anyway, because the appointment was at Caltech [the California Institute of Technology, Pasadena].

I first met Ed Stolper in 1976, when I was persuaded, in a weak moment, to become a team leader for experimental petrology in the 3-year Basaltic Volcanism Project, an experiment in group planetary science launched by the Lunar and Planetary Science Institute. I had been earth-bound since Apollo 12, and when I inquired about good candidates for the team, I was advised that I should consider Ed Stolper. When I expressed doubts that he, as a graduate student, would surely be too busy with other things to be able to attend workshops and to generate manuscript on demand, I was then told that my team would not be considered viable without Ed Stolper. It turned out that his presence was one of the brightest aspects of my time on this project.

I moved to Caltech in 1983 and found that he was well installed. He had a flourishing laboratory, a group of students, and several programs under way. To his research on lunar rocks and meteorites he had added research on oceanic basalts and considerations of the physics of migration of basalts and other melts. He had adopted infrared spectroscopy in order to measure the amount and speciation of water dissolved in silicate melts and has since extended this research to carbon dioxide solubilities. Associated with the experimental approach was the development

of thermodynamic models for silicate melts, which led him into debates with established authorities in the field but which also led to the award of the Clarke Medal.

The fundamental characteristics of Ed's work, it seems to me, are that he becomes intrigued by a problem, but no problem that is not a fundamental one, tries to devise the best possible way to solve the problem or to constrain it, and then proceeds to get on with it. He moves without hesitation to whatever instrumentation provides the prospect of answering the questions he poses. For example, he persuaded me as chairman to contribute \$25,000 in equipment funds from our division's allocation to the Nuclear Magnetic Resonance Facility in the Chemistry Division, because this would permit him and his students to tackle some mineralogical problems of interest. The best paper in *Science* for 1984 developed from Ed Stolper's lead in a 1981 Harvard paper, concerned with melt segregation. His review of compressibility data led him to conclude that the increase in density of molten basalt as a function of pressure might bring it to values higher than that of peridotite. The critical experiment is measurement of the density of molten basalt at high pressures and temperatures, which is no easy task, but Tom Ahrens has a big gun at Caltech. Sally Rigden took up the problem for her thesis, and they devised shock wave experiments using, for the first time, a molten target instead of solid material. From the results, they determined the density of a silicate melt up to pressures equivalent to those about 700 km deep in the earth. The results provide support for the concept of a sunken komatiite ocean in early earth history.

When I arrived at Caltech, I was told that Ed Stolper was known to some as "Young Wasserburg." This is an indication of his intellectual quality, his determination, and his ability to get things done as he thinks best. If he keeps up the way he is going, his string of awards may one day match even that of "Old Wasserburg." The product of this approach to research by the right individual is certainly good science, and I proudly present my colleague Ed Stolper for this year's Macelwane Award.

Peter J. Wyllie

Response

Thank you, Peter, for that generous introduction. There will, however, I think be little

doubt given my shaky voice and hands, that I am nervous, humbled, and deeply honored to receive this award.

What I would like to do in the few minutes I have up here is to describe briefly the path that my colleagues and I have followed in the work cited by Peter Wyllie on the densities of silicate melts at high pressures. This will give me an opportunity to demonstrate how serendipitous my presence up here really is and to publicly acknowledge debts to friends and colleagues.

In the fall of 1978, I was a graduate student at Harvard in charge of a course called Sophomore Tutorial, required of all students majoring in geology who wanted to graduate with honors. The classroom part of this ungraded course consisted of a series of guest lectures by faculty members to introduce them to the students. One Thursday evening, Rick O'Connell talked to us about how phase changes could be responsible for substantial, long-lived plateau uplifts. During his talk, the idea occurred to me that igneous differentiation of the upper mantle could lead to uplift. I went away and tried to see if this would work. In the process, I found that I needed to be able to estimate the density of basaltic melt at about 30 kbar. I looked up the little that was known about the compressibility of basaltic melt, guessed at its pressure dependence, and did the calculation. I never have pursued the question of uplift related to differentiation, though I think it would be effective, because I noticed something that I thought was far more interesting: According to this simple calculation, the density of basaltic melt closely approaches and perhaps even exceeds that of coexisting mantle phases at relatively low pressures. At the time, Jim Hays, Dave Walker, and I were thinking about the migration of melt in the mantle: in particular, struggling with how, given our conclusion that melts would come out very rapidly, magma could ever be retained in the mantle. The notion that the buoyancy of melts diminishes or even reverses sign at depths of only a few hundred kilometers was a possible solution to our dilemma.

Opinions on this idea have, from the start, been highly polarized. Some say it is crazy; others have seen that if it is on the right track, our surface-based notions of igneous petrology and its role in the evolution of the earth may need to be rethought. Happily, Jim Hays and Dave Walker, my wise friends, advisors, and mentors over my long years as an undergraduate and graduate student at Harvard, were enthusiastic. Along with Brad Hager, who also received a Macelwane Award [earlier] this year, we wrote a paper in which the results of this and related calculations and speculations were presented.

My work in this area might have gone no further, except that in the summer of 1979, I moved to Caltech. It is a little-known fact, but Raymond Jeanloz and I overlapped at Caltech for about a week; there is no truth to the rumor, which I will start now, that there was a connection between my arrival and Raymond's abrupt departure. During that week, at the famous Seismo Lab coffee hours, Raymond, Tom Ahrens, and I talked about how one might go about actually measuring the densities of melts at high pressures. Many ideas were discussed, but the idea of doing it by shock wave techniques emerged. Over the next year, Tom and I began to design a re-

search project to carry out such measurements and recruited Sally Rigden, a first-year graduate student, to work with us. As with all such projects, we began by bootlegging it: Lee Silver lent us a massive induction furnace, Caltech paid for electrical hookups, Tom's grants paid for everything else. Eventually, the project got going in earnest with support from NSF, but for almost 4 years, almost nothing went right, and all sorts of previously unimaginable things went wrong. Then all of a sudden, things that didn't work before started to work, and we were on our way, and this ongoing project is one of the most exciting things that I am involved in. By the way, this latter phenomenon of nothing working and then suddenly everything working for no obvious reason has been explained to me by Julian Goldsmith as a fundamental aspect of Mother Nature's personality: At first, she derives pleasure from causing you trouble when you try to discover her secrets, but if you persist, after a while, she gets bored and gives up.

What lessons do I derive from this experience? First, you never know where your ideas may come from, so always listen, and keep an open mind. I am 100% certain that, were it not for the casual connection made during Rick O'Connell's lecture that led me unpredictably to do a simple back-of-the-envelope calculation, I would *never* have come up with the idea and experimental follow-up to it that has been so fruitful and has occupied so much of my time. If you are here, Rick, I am deeply grateful.

Second, don't worry if a lot of people think that what you are doing is crazy. The testing of crazy ideas can lead to important data and insights, and sometimes the ideas might even turn out to be right. In fact, I am convinced that if you can arrange it, you should work only on problems that everyone else thinks are crazy. This will give you the breathing space to do a thorough job, and by the time the rest of the world notices that it wasn't so crazy after all, you will be very far ahead.

Third, sometimes great patience is required in the pursuit of technically difficult experiments. We experienced almost 4 years of frustration before our first successful measurement. Funding agencies must, in particular, find ways to support challenging, difficult experiments that don't yield "quick fixes."

Fourth, my colleagues at Caltech have been essential to the success that I have achieved there. I do not know where I would be in my work were it not for Tom Ahrens's enthusiasm, expertise, and willingness to collaborate and share with me. These qualities are, in my opinion, widespread among the Caltech senior faculty, and Sam Epstein, George Rossman, Lee Silver, and Gerry Wasserburg have had equally large impacts on my work.

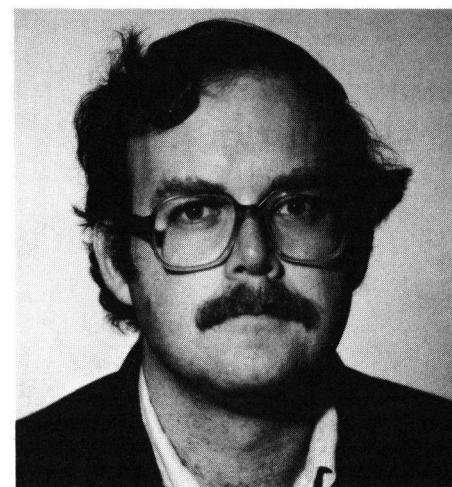
Finally, I want to paraphrase something that Sam Epstein once said to me. He said that one of the greatest pleasures of his career is the people that he has come to know through his work. In a way, we might view the work that we do as a vehicle for interacting with other people and getting to know them and how they think and to care for them. The friends that I have made through my work are as important to me as the work itself; those of you who mean a lot to me know who you are and I thank you for your help and support.

Finally, I want to tell something to my wife,

Lauren, with whom I've spent nearly half my life and with whom I have shared both our successes and failures. The point of this award, according to its originators, is to recognize scientists while they are still young so as to offer them encouragement. Lauren, I know that it doesn't seem like it at the end of a long day or when the children wake us up in the middle of the night, but it has now been certified by an august association, the American Geophysical Union, with all of its experience and national and international influence, that we are still young, so it must be so.

Ladies and gentlemen, thank you very much.

Edward Stolper



Robert A. Weller

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Citation

Robert A. Weller is a seagoing experimentalist or observational oceanographer who has made in his short career several truly outstanding contributions to understanding how the upper ocean responds to atmospheric forcing. He is without doubt one of the most capable and creative oceanographers of his age and deserves the recognition provided by the James B. Macelwane Award as a "young geophysicist of outstanding ability and promise."

After receiving his undergraduate degree in engineering and applied physics from Harvard, Bob Weller entered graduate school in physical oceanography at Scripps Institution of Oceanography and worked with Russ Davis on the development of a new mechanical current meter to be used in upper ocean studies. While several current meter designs then in use worked well on subsurface moorings in the deep ocean, no existing current meter performed well in the upper ocean. After considerable experimental effort, Bob developed a mechanical flow sensor (using two coupled propellers) with nearly perfect cosine response and then used two of these sensors mounted at right angles with associated electronics to build the vector-measuring current meter (or VMCM). By mechanically filtering out the usually very large oscillatory flows with periods less than a minute associated with surface wave and wave-induced